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With the facts that are known in regard to the clinical history of diphtheria and those which we have determined in our research, it is easy to make out a theory of the disease which reconciles all existing differences of opinion and seems to be true.

A child gets a catarrhal angina or trachitis. Under the stimulation of the inflammation products the inert micrococci in the mouth begin to grow; and, if the conditions be favorable, the sluggish plant may be finally transformed into an active organism, and a self-generated diphtheria results. It may be, however, that by appropriate treatment such a case is arrested before it fairly passes the bounds of an ordinary sore throat. Every practitioner knows that such diversity does exist. Again, conditions outside of the body favoring the passage of inert into active micrococci may exist, and the air at last become well loaded with organisms, which, alighting upon the tender throats of children, may begin to grow and themselves produce violent angina, trachitis, and finally fatal diphtheria.

In the first instance we have endemic diphtheria as we see it in Philadelphia; in the second, the malignant epidemic form of the disease as it existed in Ludington. It is also apparent that in the endemic cases the plant whose activity has been developed within the patient may escape with the breath, and a second case of diphtheria be produced by contagion. It is also plain that as the plant gradually in such a case passes from the mild to the active state, there must be degrees of activity in the contagium, one case being more apt to give the disease than is another; also that the malignant diphtheria must be more contagious than the mild endemic cases. We think there is scarcely a practitioner who will not agree that clinical experience is in accord with these logical deductions from our experimentally determined premises.

It yet remains for us to investigate as to what are the conditions outside of the body which will especially favor the production of active micrococci, and also to study the effects of agents in killing these organisms; for it is very apparent that local treatment of the throat must often be of the utmost importance, and that it will be far more effective if it be of such character as to kill the micrococci, and not simply be anti-phlogistic in its action.

SOLAR PARALLAX.

In an elaborate paper, given in full in the *American Journal of Science*, for November, Professor William Harkness draws the following conclusions:—

For convenience of reference the limiting values of the solar parallax, found by the various methods described in the foregoing pages, are presented here. It should be remarked, however, that in selecting these values the results of all discussions made prior to 1857 have been omitted; except in the case of the transit of 1761, and the smaller of the two values from the transit of 1769.

I.—Trigonometrical methods.

Mars, meridian observations	8".84	—	8".96
" diurnal observations	8.60	—	8.79
Asteroids	8.76	—	8.88
Transit of Venus, 1761	8.49	—	10.10
" " 1769	8.55	—	8.91
" " 1874	8.76	—	8.85

II.—Gravitational methods.

Mass of the earth	8".87	±	0".07
Parallactic Inequality	8.78	—	8.91
Lunar Inequality	8.66	—	9.07

III.—Photo-tachymetrical methods.

Velocity and light equation	8".72	—	8".89
Velocity and Aberration	8.73	—	8.90

To obtain a definite value of the solar parallax, it would now be necessary to form equations of condition embodying the relations between the various elements involved; to weight these equations; and to solve for it by the method of least squares. But what is the use? It is perfectly evident that by adopting suitable weights, almost any value from 8".8 to 8".9 could be obtained, and no matter what the result actually was, it would always be open to a suspicion of having been cooked in the weighting. We only know that the parallax seems to lie between 8".75 and 8".90, and is probably about 8".85. Attack the problem as we will, the results cluster around this central value. All the methods give a probable error of about $\pm 0".06$, and no one of them seems to possess decided superiority over the others. We have nearly exhausted the powers of our instruments, and further advance can only be made at the cost of excessive labor.

In the beginning of the eighteenth century the uncertainty of the solar parallax was fully two seconds; now it is only about 0".15. To narrow it still further, we require a better knowledge of the masses of the earth and moon, of the moon's parallactic inequality, of the lunar equation of the earth, of the constants of nutation and aberration, of the velocity of light, and of the light equation. All these investigations can be carried on at any time, but there are others equally important which can only be prosecuted when the planets come into the requisite positions. Among the latter are observations of Mars when in opposition at its least distance from the earth, and transits of Venus.

In 1874 all astronomers hoped and believed that the transit of Venus which occurred in December of that year would give the solar parallax within 0".01. These hopes were doomed to disappointment, and now, when we are approaching the second transit of the pair, there is less enthusiasm than there was eight years ago. Nevertheless the astronomers of the twentieth century will not hold us guiltless if we neglect in any respect the transit of 1882. Observations of contacts will doubtless be made in abundance, but our efforts should not cease with them. We have seen that the probable error of a contact observation is $\pm 0".15$, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. On the other hand, the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only $\pm 0".55$. If the sun is visible for so much as fifteen minutes during the whole transit, thirty-two negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. In view of these facts, can it be doubted that the photographic method offers as much accuracy as the contact method, and many more chances of success?

The transit of 1882 will not settle the value of the solar parallax, but it will contribute to that result, directly as a trigonometrical method, and indirectly through the gravitational methods with which the final solution of the problem must rest. As our knowledge of the earth's mass may be made to depend upon quantities which continually increase with the time, it will ultimately attain great exactness, and then the solar parallax will be known with the same exactness. Long before that happy day arrives the present generation of astronomers will have passed over to the silent majority, but not without the satisfaction of knowing that their labors will contribute to that fullness of knowledge which shall be the heritage of their successors.